Comment to Preprint wes-2024-53

Andrea N. Hahmann, DTU Wind and Energy Systems, Denmark

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Dear authors,

I have two comments regarding your manuscript:

1 Possible feedbacks from the ocean surface

In my opinion, the manuscript overlooks a crucial conceptual point that could significantly impact the manuscript results and conclusions. In the WRF model, the state of the land surface is controlled by a land surface model. Over the ocean, such a model is not often included. In your simulations, the `namelist.input` file shows that sea surface temperatures (SSTs) are specified from an input file. Using slowly varying but fixed SSTs throughout the simulation could lead to inaccuracies in calculating changes in heat fluxes and 2-meter temperatures and possibly other derived quantities. These inaccuracies result from omitting essential thermodynamic feedback processes from the ocean to the surface layer above. This factor must be included in your manuscript to ensure a fair discussion of your findings.

A possible mechanism **in nature** will be the following. In the stable case with strong winds, your results show a change in the heat from the air to the water of the order of $3\,\mathrm{W\,m^{-2}}$. This change in flux is positive downward, thus possibly increasing the skin temperature of the sea surface. This temperature change will decrease the vertical temperature gradient, which results in a reduced heat flux. The reduced heat flux could also alter the 2-meter temperature and the surface layer's stability. Thus, in nature, the ocean could respond to minimize the changes caused by wind farms. Or not. It could be argued, however, that the ocean has a large heat capacity and, thus, an excess of $3\,\mathrm{W\,m^{-2}}$ will quickly be mixed in the water column without altering the ocean's temperature. This will be linked to the stability of the ocean's surface layer. Knowing which process will dominate is only possible with measurements and accurate simulations, including the thermodynamic effect on the ocean surface. Similar processes exist in the simulation of tropical cyclones, which often consider the possible ocean surface changes.

In your article, you cite the work of Golbazi et al. (2022), which carried out similar shorter WRF model simulations with various sizes of wind turbines. From their methods and namelist, I can also see that their simulations were done with fixed (but spatially more precise) SSTs. So, their results and discussion also disregard the possible effects of the surface ocean's response to changes in surface fluxes and temperatures. So, the article cannot be used to substantiate your results.

Through the results and discussion section of your manuscript, I often find that you mix land and offshore publications. The processes are different in these two environments in nature and the models and should not be mixed.

The issue of the possible impact of fixed SSTs should be discussed and addressed in your manuscript. This discussion is crucial for the reliability of your findings. It is outside the scope of your manuscript, but WRF model simulations, including the effects of a slab ocean, are possible. Running with a fully coupled ocean will be even better but expensive.

2 Dependence of your results on PBL and wind farm parameterization

While your results are exciting, they represent only one possible scenario with one WRF PBL scheme and one wind farm parameterization. Recent publications have shown that the impact of wind farms on the atmosphere is highly dependent on the PBL and wind farm parameterization used. The paper should emphasize this point and acknowledge that observations have yet to verify most aspects of the simulated impacts of large wind farms on the atmospheric flow.

Statements like the one in the abstract, "Offshore wind energy projects are currently in development off the east coast of the United States and will likely influence the local meteorology of the region." should be avoided. It is all a question of degrees and assessment of significance.